Health Care Reform - Transfer Program or Something More? Evidence from Mexico's Seguro Popular Program

Author:
Melissa Knox, Ph.D.
University of Washington
Department of Economics
305 Savery Hall
Box 353330
Seattle, WA 98195 USA
Phone: 1 (206) 543-5835
Fax: 1 (206) 685-7477
knoxm@uw.edu

Running title: Medium Term Impacts of Seguro Popular
Keywords: Health Systems Reform; Mexico; Seguro Popular

Acknowledgements: Partial support for this research came from a Eunice Kennedy Shriver National Institute of Child Health and Human Development research infrastructure grant, R24 HD042828, to the Center for Studies in Demography & Ecology at the University of Washington.

Abstract:
This paper examines the impact of Mexico's *Seguro Popular*, or "People's Insurance", five years after program introduction. Seguro Popular is a major health system reform introduced between 2002 and 2013. The program provides free or subsidized health insurance to Mexican families not covered by formal social security programs, which is nearly 50 percent of the population. Most previous studies of the program have found little to no short-term effect of program participation on health care utilization and health, but have found a decrease in catastrophic health spending among affiliates. This analysis uses panel data spanning the years 2002 through 2009, and contains results for urban individuals enrolled in the program for up to five years. Using a stepped wedge study design, I find both a significant and large increase in the likelihood of using a public clinic for enrolled children and an increase in the total number of health care visits for adult men in the program. These results only appear five years after program affiliation. In contrast to previous results, I also find health improvements – a result that appears to be driven by children under ten and adult women.

1. INTRODUCTION

Mexico's *Seguro Popular* universal health insurance program, introduced in 2002, provides a new opportunity to measure the impact of a large-scale health insurance reform in a developing or middle income country. *Seguro Popular* (translated as either People's or Popular Insurance and abbreviated as SP) was designed to provide comprehensive health insurance coverage to the 50 million Mexican who were at that time not affiliated with the formal employment sector and therefore not entitled to formal social security. The program was targeted to the poor, indigenous, and uninsured, with a goal of achieving universal health insurance coverage (albeit in a two tiered system) and reducing catastrophic spending on health. Previous research has found robust evidence of protective financial impacts of the program, but health impacts are less clear. Understanding the role of SP in promoting population health and reducing health disparities can provide needed insight into the impact of similar reforms, especially in developing countries. It can also help to identify the importance of health care reforms in a broader aid context that now includes unconditional cash transfers, which have recently been shown to promote health in women and children (Amarante et al., 2011) and evidence of other large scale reforms that show little to no long term health impacts (Baicker et al., 2013; Camacho & Conover, 2013).

In this paper, I perform the first measurements of SP's impact on health and health care utilization for both adults and children five years after introduction and controlling for adverse selection into the program. I take advantage of the natural experiment created by the geographically staggered roll-out of program availability across Mexico to look at these medium-term impacts on a panel of individuals. This paper builds on previous work that experimentally evaluated a pilot study of SP, but only ten months after program introduction (King 2009). Due to the lack of randomized evaluation data past the ten-month time frame, others have used the geographic roll-out of the program to study program impacts in panel data using difference in differences (Barros 2008; Knox 2008). Still, these papers only look at impacts after the first year or two after program introduction, and find few effects on health or utilization. Others have investigated medium-term impacts, but have either had to use cross-sectional data (Rivera-Hernandez et al. 2016; Sosa-Rubi et al. 2009) or have looked at region-level outcomes (Conti & Ginja 2016; Turrini et al. 2016).

This study combines administrative data on the roll-out of SP with a unique dataset that includes a panel of individuals and spans seven years around the introduction of the program, from 2002 to 2009. To reduce bias from endogenous selection into SP, I select both the treatment and control group from among the set of households that all choose to enroll in SP as soon as it becomes available to them. This is similar to the stepped wedge study design used in the medical literature to correct for endogeneity in medical treatment take-up.

My main findings are an increase in utilization of health clinics (including newly-built SP facilities) for children and adult men, and health improvements for children and adult women. Both sets of results are observed for individuals who have been affiliated with SP for five years. These are the first results to show both health and utilization improvements in the medium term using panel data.

The remainder of the paper is as follows: I present a brief history of health reform in Mexico and review previous studies of SP in Section 2, Section 3 describes the data, Section 4 describes the empirical strategy, and Section 5 describes the results. Section 6 discusses the findings and concludes.

2. BACKGROUND ON *SEGURO POPULAR*

Mexico introduced the *Seguro Popular* health insurance program in 2002 as part of a broader reform of their health care system. SP provided what was essentially free health insurance coverage for a broad range of treatments and diseases (Knaul et al., 2012). Before this reform, only about half of Mexico's 100 million people had access to low cost care through formal social security benefits, which included access to health care facilities run by either the Mexican Social Security Institute (*Instituto Mexicano del Seguro Social* or IMSS)[1] or the Institute for Social Security and Services for State Workers (*Instituto de Seguridad y Servicios Sociales de Trabajadores del Estado* or ISSSTE). Among the poor, rates of protection were even lower, with 7 percent and 25 percent social security coverage for workers in income quintiles 1 and 2, respectively (Rofman et al., 2008). The approximately 50 million Mexicans without coverage, then, were forced to find care at Ministry of Health (*Secretaria de Salubridad y Asistencia* or SSA) facilities, from private providers, or to forgo medical care altogether.

This lack of health insurance for the unemployed, self-employed and those in the informal (untaxed) employment sector contributed to high levels of out of pocket spending on health care (Frenk et al., 2006). Estimates from that time show that between two and four million Mexican households were experiencing catastrophic medical spending every year (Frenk et al.,

---

[1] Formal social security services also includes life insurance, disability pensions, work-risk pensions, retirement pensions, sports and cultural facilities, day care, and housing loans. They are paid for through payroll taxes and government financing and are not optional.

2006), and the bulk of household health spending by the poorest quintile was going toward medicines and ambulatory care, not hospitalizations (Knaul et al., 2006). Inequities in access to care were also correlated with large regional and ethnic health disparities in Mexico. The intersection of these disparities lead to states with large indigenous populations like Guerrero having both great deficits in access to care and worse health outcomes, especially for women and children. (Gutiérrez et al., 2002).

SP was introduced in stages. Out of 32 states, 5 joined the program in 2002, 17 joined in 2003, 7 more joined in 2004, and the final 4 states were covered in 2005 and 2006. Each state then made the decision to either remodel old SSA facilities or building new facilities to be used by SP affiliates. Once the facilities were accredited by the SP administration, states were able to begin affiliating their citizens at the level of the *municipio* (or district). Coverage within states was variable and the roll-out extended through 2009. Overall, the goal was to affiliate about 14% of the population per year, and by 2012, there were 12 million affiliated families (52 million people) (Knaul et al., 2012). This variation at the *municipio* level forms the basis of this paper's analysis.

In spite of its large scale, there is little conclusive evidence of the health and utilization impacts of SP. A short-run experimental evaluation found little impact of the program on health or utilization (King et al., 2009; Spenkuch, 2012). Of the studies performed on cross-sectional data, some show potential health impacts (Sosa-Rubí, Galárraga, and Harris, 2009), while others do not (Rivera-Hernandez et al., 2016). These studies are not able to fully control for unobserved sources of demand for health insurance, however. A stronger case can be made that SP does reduce out of pocket health care expenditure catastrophic medical spending, with multiple studies consistently confirming these findings, especially for urban affiliates (Galárraga et al., 2010; Garcia-Diaz & Sosa-Rubi, 2011; Grogger et al., 2014).

Given the resources devoted to the SP program and those like it, a clearer picture of program impacts should answer the question of whether SP is simply a financial protection program with the potential for affiliates to engage in *ex ante* moral hazard, or whether it can be credited with improving health and creating the economic benefits that are often associated with health improvement (Strauss & Thomas, 1998).

3. DATA

The analysis is performed on a 4,229 person sample of individuals in families that gained access to SP between 2004 and 2009. The families are drawn from the evaluation survey of the urban expansion of the Oportunidades conditional cash transfer program, called Encuesta de Evaluación de los Hogares Urbanos (or ENCELURB). The survey was conducted annually in 2002, 2003, and 2004 and again in 2009. Over 150 urban *municipio*s were represented in the full ENCELURB data set (Behrman, Gallardo-García, Parker, Todd, & Vélez-Grajales, 2012). These *municipios* were selected based on perceived need of residents, so all households in the current study are from poor and urban districts.

To determine when individual municipalities, and thus the families living within them, became eligible for SP, this data set was combined with official enrollment data obtained from the *Seguro Popular* Administration in Mexico City (frequently called the Padrón). Figure 1 shows the total affiliated families in the fourth quarter of every year between 2002 and 2009, drawn from this data set. For the purposes of this study, ENCELURB families were sorted into early adopter and late adopter groups. As described in Section 4, the early adopters lived in *municipio*s with a SP facility accredited in 2004 and the late adopters lived in *municipio*s that were accredited by SP between 2007 and 2009. The early adopters represent the treatment group for the medium term estimates of program effects.

[Figure 1 here]

Mean values of the characteristics of the 4,229 individuals in the early adopter (N=3473) and late adopter (N=756) groups are given in Table 1 along with their standard deviations. Table 2 contains statistics from 2002 baseline demographic characteristics and utilization measure, before both sets of families gained access. Table 1 reports that individuals that were treated in 2004 had significantly fewer health care visits in the month before the survey, reported fewer sick days in that same month (although there were no significant differences in a more objective measure of health), were slightly less educated, and less likely to have Oportunidades and health insurance in 2002. While it is not ideal that there are several ways in which individuals in the two treatment groups differ, the differences do not suggest that early adopters were sicker or had higher demand for health care. In fact, the late adopter group's increased access to Oportunidades, which was also being introduced to urban areas at this time, could explain most if not all of the observed differences in outcomes in 2002.

[Table 1 here]

4. EMPIRICAL STRATEGY

4. 1 The Natural Experiment and Stepped Wedge Study Design

This study relies on the staggered geographic roll-out of SP to identify treatment effects. Finding unbiased treatment effects with natural experimental methods requires that the timing of program introduction is not correlated with the unobserved characteristics of program affiliates that impact program demand, either at the family or *municipio* level. I address both of these concerns in turn.

At the *municipio* level, the main concern with potential endogeneity in the introduction of *Seguro Popular* is the concern that individuals in *municipio*s that are accredited early are moving along a different outcome path over time than individuals in late accrediting *municipio*s. This is the assumption made by previous studies of financial and labor market impacts of the program (Bosch & Campos-Vazquez, 2014; Galárraga et al., 2010; Sosa-Rubí et al., 2009), which have treated the timing of the roll-out as exogenous. Additionally, Barros (2008) and Knox (2008) discuss the political, but not health-related, motivations behind the timing of affiliation. Recent studies, such as Rivera-Hernandez and co-authors, find that lower population density *municipio*s were affiliated earlier in the program's introductory period for political reasons (2016). They treat the timing of program introduction as independent of health and utilization outcomes (Rivera-Hernandez et al., 2016)[2].

As stated in Section 3, the *municipio*s in the present study were all drawn from poor urban *municipio*s that were selected for their eligibility for the Oportunidades cash transfer program. This homogeneity in *municipio*s makes it less likely that there are unobserved

[2] Turrini and co-authors (2016), however, disagree with this conclusion. They argue that the roll out was not random and that the municipalities that received access to SP first were more highly educated, had higher expenditures, and higher rates of formal employment. In the data used in the current study (discussed above), the opposite appears to be true.

characteristics that may determine whether a *municipio* was in the early or late adopter group and also drive trends in outcomes such as health care utilization or health. To further test this hypothesis, an analysis of pre-trends in the outcomes is presented in the paper's appendix. This analysis shows no significant differences in trends between individuals in early and late treated *municipios* in the current study.

At the family level, bias in estimated effects may come from adverse selection into the program. To combat this bias, I use a modified stepped wedge research design. The traditional stepped wedge research design rolls out a program or intervention to randomly selected sub-groups of trial participants (either individuals or clusters) over time, until all participants have access to the program. The outcome data used in this study were only available in 2002, 2003, 2004 and 2009, and so *municipio*s given access in 2004 were considered to be one cluster and those given access between 2007 and 2009 a second cluster. The former are considered "early adopters" while the latter are "late adopters." It is also important to note that only families that chose to affiliate as soon as they were legally able to do so are included in the clusters. Therefore, all individuals in the study are the type who affiliate to the program the first year it is offered to them. This design also allows short term (0-2 year) and medium term (5 year) treatment effects to be estimated separately.

[Table 2 here]

The logistics of the research design are shown in Table 2, with 0 identifying clusters that have not yet been treated, T1 identifying the time and cluster for which the short term treatment effect is measured, and T2 identifying the time and cluster for which the five year treatment effect is measured. Figure 2 shows the total number of affiliated families by year in the *municipio*s available in the ENCELURB study that fit the criteria of either early or late adopters. This total is from the Padrón, not from the ENCELURB sample[3].

[Figure 2 here]

An important identifying assumption in stepped wedge research design is that all of the study subjects are similar in unobserved determinants of the outcomes measured, including their demand for the program (Hussey and Hughes 2007). This identifying assumption is the same as the exogeneity assumption used in the previous studies addressed above (e.g. Gallaraga et al. (2010)).

4.2 Empirical Models

Several of the outcomes measured are dummy variables that have a value of one if a service was utilized in the past month, while others are counts of events. For the probabilistic outcomes, the likelihood of an individual choosing to use a health care service or receive a test is determined by a latent variable Y* that satisfies Equation 1

$$Y_{ijt}^{*} = \alpha + \beta_1\left(T_{04,ij} * Yr_{04,t} + T_{07-09,ij} * Yr_{09,t}\right) + \beta_2\left(T_{04,ij} * Yr_{09,t}\right) \qquad (1)$$

$$+\beta_3 T_{04,ij} + \pi Z_{ijt} + \gamma_i + \mu_1 Yr_{04,t} + \mu_2 Yr_{09,t} + \varepsilon_{ijt}$$

The treatment indicators are the T_{ij}s, which indicate that the individual is eligible for treatment in a given year (either 2004 or 2007-2009) and chooses to affiliate in that year. These are

[3] Appendix table A1 gives the state, *municipio* and number of families for each ENCELURB cluster included in the present study.

interacted with dummy variables for the years 2004 and 2009, so those in the early adopter group will have two treatment effects: one for the short term (β_1) and one for the medium term (β_2), while those in the late adopter group will only have one (short term) treatment effect (β_1)[4]. The main variable of interest in this study is β_2, the medium term impact of affiliation on the outcomes of interest for the early adopter group.

The outcomes Y_{ijt},* are determined for each individual i, in *municipio* j, and year t as a function of being affiliated with *SP* either in the short or medium term, and living in the group of *municipio*s accredited and admitted to the program in 2004 (the early adopters) (β_3). Additionally, outcomes are determined by a vector of household and individual characteristics such as sex, age, indigenous status, and household size (π), individual random effects (γ), and year fixed effects (μ). The controls for household characteristics also include the family's status in the Oportunidades program, which has a strong correlation with the individual's use of preventive care. The results presented can be viewed as impacts of SP net of the impact of Oportunidades on household behavior. For the reasons discussed in Section 4.1, I assume that treatment group fixed effects control for baseline differences in outcome and $\varepsilon_{ijt} \sim N(0, \sigma^2)$.

Since only utilization choices are observed and not probabilities, the actual effects of treatment and other control variables on the probability of the outcome are estimated using a Probit model. Two of the other outcomes measured in the ENCELURB data – number of days of illness and number of health care visits – are counts of events. Following Cameron and Trivedi

[4] Alternative specifications with separate short term effects for early and late adopters were also estimated. The medium term results were not materially different from those reported here.

(2013), I model the data as the result of a Poisson process. The treatment effects and other control variable definitions are as above in Equation 1[5].

4.3 Calculating Treatment on Treated Effects from Non-Linear Regression Estimates

Treatment on treated (TOT) effects are the marginal impact of moving from a state of being untreated in the post treatment period to a state of being treated, while holding all other variables constant. In a standard linear model difference in differences estimation, the TOT impact would be equal to the estimated coefficients on the interaction terms (here, β_1 and β_2). However, in this case, the treatment effects are the differences in the outcomes of the limited dependent variable, Y, not the latent variable Y*. In the non-linear Probit and Poisson models used in the present study, the interaction effect does not equal the marginal treatment effect, and the statistical significance of the treatment effect cannot be inferred from the standard errors calculated from the interaction term coefficient (Ai and Norton 2003; Puhani 2012).

In other words, the estimated coefficients β_1 and β_2 will not be directly translatable into the true estimates of the impact of *SP* on the likelihood of the outcomes of interest among the treated. Instead, following Karaca-Mandic and co-authors (2011), I calculate the marginal treatment effect by calculating the difference between estimated value of the latent variable (from equations 1 and 5 above) for those in the treated group in the post treatment period and for those in the treated group in the post treatment period, assuming that they did not receive

[5] Huber-White heteroskedasticity consistent estimates of standard errors are used to correct for unequal means and variances of the outcome data following Cameron and Trivedi (2013).

the treatment (Karaca-Mandic et al. 2011). For example, the medium term treatment effect can be calculated from

$$\widehat{TE} = E\left[F\left(\hat{\alpha} + \widehat{\beta_2} + \widehat{\beta_3} + \hat{\pi}Z_i + \widehat{\mu_2}\right) - F(\hat{\alpha} + \hat{\pi}Z_i + \widehat{\mu_2})\right] \qquad (6)$$

Where F(▯)represents the nonlinear function used in the original estimation and hats represent estimated values. The treatment effect is then the mean of the individual-specific predicted values. This treatment effect is marginal in the sense that it is only valid for one specific value of the covariates for each individual in the sample, but it is averaged over every individual.

The variance of the treatment effect for each individual in the sample, $\hat{\sigma}_{iTE}^2$, must also be predicted using the delta method to approximate a marginal value for each person in the sample. The final reported value is the average of these marginal values.

$$\sigma_{TE} = \sqrt{E[\hat{\sigma}_{iTE}^2] + Var(\hat{\sigma}_{iTE}^2)/N]} \qquad (7)$$

6. RESULTS

6.1 Health Care Utilization

To find the effect of *SP* on health care consumption, I looked at the change in the probability of an individual visiting a health care clinic, which is the most likely type of visit to be affected by SP affiliation. The impact of SP on having any type of outpatient health care visit is also reported. This variable is an indicator not only for clinic visits, but visits to pharmacists, traditional healers, specialists, nurses, and outpatient hospital visits. These impacts were all estimated using the Probit model with the probabilities as determined in Equation 1. The impact of *SP* on the total number of visits reported in a month is estimated using the Poisson model with the control variables from Equation 1. In keeping with the discussion in Section 4.3, the calculated medium term treatment effects and standard errors are reported in table 3[6]. Together, these coefficients show whether affiliates are increasing their utilization of all health care services or only substituting toward clinic usage but not increasing overall utilization.

[Table 3 here]

Table 3 shows that five years after program introduction, there is no statistically significant evidence of a change in the likelihood of either clinic utilization or any utilization, and there is no increase in the total number of visits for the full sample. Because of the existence of Oportunidades and its focus on preventive care for children and, to some extent, women, I anticipate that the effects of SP may be heterogeneous for these subgroups. Therefore, I also estimate separate regressions for children under ten (in 2002), adult women, and adult men. The total number of individuals over 40 in this sample is less than 300, making it difficult to separately estimate impacts on older adults.

Children under ten are 5 percentage points (a 62% increase over 2002) more likely to have visited a clinic in the past month. Without evidence of increased utilization overall, though, I can't reject that families are substituting toward clinic usage and away from other forms of health care for their children. Adult men who were at least ten in 2002 also show a significant increase in utilization in the medium term, with an average increase of 0.04 health visits per month (a 40% increase over 2002 utilization)[7]. There is no evidence of increased clinic

[6] The estimated coefficients from the probit model for clinic usage and total health visits are reported in tables A2 and A3, respectively.

[7] Because the sample was split, there could be concern that the subgroup level results suffer from the problem of multiple inference. For this reason, the number of subgroups was kept low, and only subgroups with an *a priori* theoretical reason to have different outcomes were tested. A common, but conservative, correction for

usage for this group, however, so they may be using other sources of health care than the SP accredited clinic.

6.2 Health Outcomes

With the limited increase in consumption of health care services demonstrated above and concerns about moral hazard, it becomes relevant to ask whether affiliation with *SP* leads to improved health status. I look at two measures of health status available in the ENCELURB survey. The first is a measure of self-reported illness, similar to measures of general health status used in many other studies. The families are asked to report the number of days that each family member was ill in the past month. The second measure of health asks the number of days that the respondent was unable to perform his or her normal activities due to illness. Both measures can suffer from reporting bias, but the second is more objective than the first, so I expect to see more effects of SP on this outcome (Gertler, Rose, and Glewwe 2000).

I estimated Equation 1 using a Poisson model and the two measures of health status described above as outcomes. Table 4 shows the marginal impacts of *SP* in the medium term on these two measures of health status for the full sample and several subsamples divided by

---

performing multiple significance tests is the Bonferroni method, which requires that the observed p value of a coefficient of one significance test be multiplied by k, the number of tests. If the critical value for significance is α, then the test only meets the criteria for significance if $kP < \alpha$ (Bland & Altman, 1995). In table 4, k is 3 and there are two significant outcomes, the increase in clinic use for children (p=0.09) and the increase in total visits for men (p=0.0002). Only the result for men passes the Bonferroni test for α=0.10. For this reason, the result for children's utilization should be considered suggestive rather than definitive. Given the health improvements observed for children, however, it is highly possible that there is a true effect under the statistical noise.

age and gender[8]. There is no statistically significant change in the number of self-reported sick days experienced in the past month for the full sample or any subgroup.

Using an individual's reported inability to perform normal activities as a proxy for days of illness, however, shows a different story. The full sample, women and children show significant health improvements in the medium term using this measure. For the full sample, the decline in the average number of days sick per month is 0.33 (a 34% decrease in sick days in 2002). When the analysis is broken down by age and sex, only adult women over 10 and children under 10 see significant health improvements after five years of affiliation. Women see a decline of 0.45 days per month (a 53% decrease from 2002 averages), while children see a decline of 0.53 days per month (a 155% decline from their 2002 average). No health improvements are measured for men[9].

[Table 4 here]

7. DISCUSSION AND CONCLUSIONS

There is great potential for large scale health system reforms such as *SP* to return government investment both through lowering out of pocket spending for families and through directly improving population health and reducing health disparities. Additionally, the income effect of such a savings has the potential to lead to an improvement in educational and employment outcomes for affected families with the secondary health impacts that these

[8] Poisson regression results are shown in table A3.

[9] As in the utilization section, I correct here for multiple inference tests. The medium term health improvements for children (p=0.03) and women (p=0.02) both meet the criteria for significance at α=0.10.

improvements bring. However, there is much competition from other social programs, including conditional and unconditional cash transfers, so it is fitting that advocates for health reforms to show that these programs can provide something more than just an increase in household budgets. This paper seeks to find such evidence of increased utilization and health improvements in order to supplement previous findings of decreased household health spending due to *SP*. The measured health and utilization improvements distinguish the program from other options available to policy makers.

This paper presents the first evidence of increased utilization and improved health for *SP* affiliates in the medium-term using panel data and controlling for selection into the program. I find a significant increase in the probability of children and adult men utilizing health care services. If we believe that the individuals in the study were not paying for any medical services after they affiliated with SP (i.e. that all reported care was provided in a SP facility), then the 40% increase in male utilization following a 100% decrease in the cost of medical care can be interpreted as price elasticity of demand for medical care of -0.4. This is larger than the commonly used U.S.-based value of -0.2 found in the RAND health insurance experiment (Manning et al., 1987). This increased utilization does not translate to health improvements for adult men, however. Only children see both increased utilization and health improvements, in spite of the fact that this population was already well-served for preventive care by programs such as the conditional cash transfer program, Oportunidades. This result complements the reductions in infant mortality detected by Conti and Ginja (2016), who found regional increases in hospital admissions as a result of SP.

The only other sub-group that shows health improvements, adult women, shows no evidence of changing their utilization behavior. It is possible that the health improvements for this group are the result of increased non-health consumption due to the income effects of the program, that women's health improvements are spillovers from caring for healthier children, of the ENCELURB survey does not capture forms of health care utilization most significant for women, such as hospitalizations. Meanwhile, increased utilization for men leads to no health improvements, suggesting that moral hazard might be at work in this case to produce excessive utilization of health care.

Overall, this study provides some evidence to support cross sectional studies that find that *SP* improves health, and is acting as more than just financial protection for poor Mexican families, contrary to findings from similar health care expansions to the poor (Baicker et al., 2013). The appearance of impacts only after five years of program affiliation suggests that either the early stages of the program did not provide sufficient coverage to induce changes in health care utilization, the quality of health care was initially poor, or queuing or some other form of barrier to access was reducing access during this period. These results provide a new understanding of previous short-term studies of *SP* that find no change in utilization and a decrease in preventive visits.

A major limitation of the results here is that the sample studied is drawn from urban areas alone. Given the evidence that there is heterogeneity in health savings due to *SP* between rural and urban areas (Grogger et al., 2014), the results presented in this study may not hold for rural areas with less access to exclusive *SP* clinics. These results might, instead, be viewed as upper bounds on the health and utilization impacts of *SP* on the universe of affiliates.

However, they can also be viewed as indicative of the health gains that can be achieved in both rural and urban areas as investment in the program progresses. They suggest that a thorough health system reform that fully insures all citizens against the bulk of a country's burden of disease has the potential to improve both health and financial outcomes.

7. REFERENCES

Amarante, V., Manacorda, M., Miguel, E., & Vigorito, A. (2011). Do Cash Transfers Improve Birth Outcomes? Evidence from Matched Vital Statistics, Social Security and Program Data, *8*(2), 1–43.

Autor, D. H. (2003). Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing. *Journal of Labor Economics*, *21*(1), 1–42. http://doi.org/10.1086/344122

Baicker, K., Taubman, S. L., Allen, H. L., Bernstein, M., Gruber, J. H., Newhouse, J. P., … Finkelstein, A. N. (2013). The Oregon Experiment — Effects of Medicaid on Clinical Outcomes. *New England Journal of Medicine*, *368*(18), 1713–1722. http://doi.org/10.1056/NEJMsa1212321

Barros, R. (2008). Wealthier but not much Healthier: Effects of a Health Insurance Program for the Poor in Mexico. *Stanford University*, (November), 1–46. Retrieved from http://economics.stanford.edu/files/JMP_RBarros.pdf

Behrman, J. R., Gallardo-García, J., Parker, S. W., Todd, P. E., & Vélez-Grajales, V. (2012). Are Conditional Cash Transfers Effective in Urban Areas? Evidence from Mexico. *Education Economics*, *20*(3), 233–259. http://doi.org/10.1080/09645292.2012.672792

Bland, J. M., & Altman, D. G. (1995). Multiple Significance Tests: The Bonferroni Method. *Source BMJ: British Medical Journal*, *310*(21). Retrieved from http://www.jstor.org/stable/29726097

Bosch, M., & Campos-Vazquez, R. M. (2014). The Trade-Offs of Welfare Policies in Labor Markets with Informal Jobs: The Case of the "Seguro Popular" Program in Mexico. *American Economic Journal: Economic Policy*, *6*(4), 71–99. http://doi.org/10.1257/pol.6.4.71

Camacho, A., & Conover, E. (2013). Effects of Subsidized Health Insurance on Newborn Health in a Developing Country. *Economic Development and Cultural Change*, *61*(3), 633–658. http://doi.org/10.1086/669263

Cameron, A. C., & Trivedi, P. K. (2013). *Regression Analysis of Count Data* (2nd ed.). Cambridge: Cambridge University Press2. Retrieved from http://faculty.econ.ucdavis.edu/faculty/cameron/racd2/

Conti, G., & Ginja, R. (2016). Health Insurance and Child Health: Evidence from Mexico. Retrieved from http://ftp.iza.org/dp10122.pdf

Frenk, J., González-Pier, E., Gómez-Dantés, O., Lezana, M. A., & Knaul, F. M. (2006). Comprehensive reform to improve health system performance in Mexico. *Lancet (London, England)*, *368*(9546), 1524–34. http://doi.org/10.1016/S0140-6736(06)69564-0

Galárraga, O., Sosa-Rubí, S. G., Salinas-Rodríguez, A., & Sesma-Vázquez, S. (2010). Health insurance for the poor: impact on catastrophic and out-of-pocket health expenditures in

Mexico. *The European Journal of Health Economics: Health Economics in Prevention and Care*, *11*(5). Retrieved from https://escholarship.org/uc/item/0mn1g2rq#page-1

Garcia-Diaz, R., & Sosa-Rubi, S. G. (2011). Analysis of the distributional impact of out-of-pocket health payments: evidence from a public health insurance program for the poor in Mexico. *Journal of Health Economics*, *30*(4), 707–18. http://doi.org/10.1016/j.jhealeco.2011.04.003

Gertler, P. J., Rose, E., & Glewwe, P. (2000). Designing Household Survey Questionnaires for Developing Countries. In M. Grosh & P. Glewwe (Eds.), (First, pp. 177–216). Washington D.C.: The World Bank.

Grogger, J., Arnold, T., Leon, A. S., & Ome, A. (2014). Heterogeneity in the effect of public health insurance on catastrophic out-of-pocket health expenditures: the case of Mexico. *Health Policy and Planning*, *30*(5), 593–599. http://doi.org/10.1093/heapol/czu037

Gutiérrez, J. P., Barraza-Lloréns, M., Bertozzi, S., & González-Pier, E. (2002). Addressing Inequity In Health And Health Care In Mexico. *Health Affairs*, *47*(213), 47–56. http://doi.org/10.1377/hlthaff.21.3.47

King, G., Gakidou, E., Imai, K., Lakin, J., Moore, R. T., Nall, C., … Llamas, H. H. (2009). Public Policy for the Poor? A Randomised Assessment of the Mexican Universal Health Insurance Programme. *The Lancet*, *373*. Retrieved from http://gking.harvard.edu/files/abs/spi-Abs.shtml

Knaul, F. M., Arreola-Ornelas, H., Méndez-Carniado, O., Bryson-Cahn, C., Barofsky, J., Maguire, R., … Sesma, S. (2006). Evidence is good for your health system: policy reform to remedy catastrophic and impoverishing health spending in Mexico. *Lancet (London, England)*, *368*(9549), 1828–41. http://doi.org/10.1016/S0140-6736(06)69565-2

Knaul, F. M., González-Pier, E., Gómez-Dantés, O., García-Junco, D., Arreola-Ornelas, H., Barraza-Lloréns, M., … Frenk, J. (2012). The quest for universal health coverage: achieving social protection for all in Mexico. *Lancet*, *380*(9849), 1259–79. http://doi.org/10.1016/S0140-6736(12)61068-X

Knox, M. (2008). Health Insurance for All: An Evaluation of Mexico's Seguro Popular Program. *American Economic Society Annual Meeting Papers*, (November), 1–51. Retrieved from http://www.google.com/search?client=safari&rls=en&q=Knox+Health+Insurance+for+all+an+evaluation&ie=UTF-8&oe=UTF-8\npapers2://publication/uuid/3D5F199E-D52E-4A7B-B7E4-8AF00EF3F575

Manning, W. G., Newhouse, J. P., Duan, N., & Keeler, E. B. (1987). American Economic Association Health Insurance and the Demand for Medical Care : Evidence from a Randomized Experiment Published by : American Economic Association Stable URL : http://www.jstor.org/stable/1804094 Your use of the JSTOR archive indicates yo, *77*(3), 251–277.

Rivera-Hernandez, M., Rahman, M., Mor, V., & Galarraga, O. (2016). The Impact of Social Health Insurance on Diabetes and Hypertension Process Indicators among Older Adults in Mexico.

Health Services Research, *51*(4), 1323–1346. http://doi.org/10.1111/1475-6773.12404

Rofman, R., Lucchetti, L., Ourens, G., Bertranou, F., Bucheli, M., Cordero, I., … Gonzalez, F. S. (2008). Pension Systems in Latin America: Concepts and Measurements of Coverage.

Sosa-Rubí, S. G., Galárraga, O., & Harris, J. E. (2009). Heterogeneous impact of the “Seguro Popular” program on the utilization of obstetrical services in Mexico, 2001-2006: a multinomial probit model with a discrete endogenous variable. *Journal of Health Economics*, *28*(1), 20–34. http://doi.org/10.1016/j.jhealeco.2008.08.002

Spenkuch, J. L. (2012). Moral hazard and selection among the poor: evidence from a randomized experiment. *Journal of Health Economics*, *31*(1), 72–85. http://doi.org/10.1016/j.jhealeco.2011.12.004

Strauss, J., & Thomas, D. (1998). Health, Nutrition, and Economic Development. *Journal of Economic Literature*, *36*(2), 766 – 817. http://doi.org/http://www.aeaweb.org/jel/index.php

Turrini, G., Farfán, G., Genoni, M., Thomas, D., & Velasquez, A. (2016). *Causal effects of universal health insurance : Evidence on child health in Mexico*.

Appendix 1: Examining Pre-Trends and Identification of Treatment Effects

Following Autor (2003), I test the validity of the assumption that there is no difference in trends in outcome variables between the *municipios* treated in 2004 and those treated in 2007-2009 by estimating models 3 and 5 above with treatment leads. Specifically, I include a dummy variable for the one year treatment leads. Since there are baseline differences in some of the outcome variables, I include a dummy variable that is always one for households in early adopter *municipio*s. I also include the standard control variables and dummy variables for all years in the study. The latent variable y* is then modeled to follow the functional form:

$$Y_{ijt}^{*} = \alpha + \beta_0 * T_{04,i} + \beta_{T-1}\left(T_{04,i} * Yr_{03,t}\right) + \beta_T\left(T_{04,i} * Yr_{04,t}\right) + \beta_{T+5}\left(T_{04,i} * Yr_{09,t}\right)$$

$$+\pi Z_{ijt} + \gamma_i + \mu_1 Yr_{02,t} + \mu_2 Yr_{03,t} + \mu_3 Yr_{04,t} + \mu_4 Yr_{09,t} + \varepsilon_{ijt} \qquad (2)$$

The coefficients are estimated using either a Probit model for binary outcomes such as clinic usage and any health care in the last 30 days, or are assumed to follow a Poisson process for count outcomes such as total health care visits or total days of illness in the last 30 days. I interpret the estimate of β_0 to be the level differences in outcomes between the early and late adopter groups, measured in 2002. The estimated coefficient for the pre-treatment lead, β_{T-1}, indicates whether there were changes in outcomes happening over time before the treatment was introduced to the early adopter group. If this coefficient is indistinguishable from zero, then there is no evidence that outcomes were changing in advance of the introduction of *Seguro Popular* and status in the late adopter group can be controlled for by including a treatment group dummy, as is done in Equation 1.

I perform the analysis of pre-trends for the four outcomes that seem to show the most impact from *Seguro Popular* affiliation: any clinic visit in the last month, any health care visit in the last month, total number of health care visits in the last month, and days unable to complete normal activities in the last month. For each of these outcomes, the results are shown for the full sample of individuals, children under ten, adult women and adult men. Figure A1 shows pre-trends in clinic usage, Figure A2 shows pre-trends in total health care utilization, Figure A3 shows pre-trends in total number of health care visits, and Figure A4 shows pre-trends in reported days of illness due to inability to perform usual activities. Several estimates of the effect of being in the treatment group are significant, confirming that there are persistent differences in levels of outcomes between the early and late adopter groups.

However, the estimates of the one year treatment lead are small and have large standard errors, showing no evidence that a differential trend exists in the early adopter group for these outcomes. The graphs also show that most of the coefficients for the year of treatment are also statistically indistinguishable from zero, indicating that no differential trends exist until individuals have been receiving treatment for many years. The full regression results, along with the coefficient controlling for level differences between early and late adopter groups are found in tables A7 through A10.

8. TABLES

Table 1 – Summary statistics in 2002 by treatment year

	2002 Control Variables			
	Late Adopters (N=756)		Early Adopters (N=3473)	
	Mean	Std. Dev.	Mean	Std. Dev.
Sex (1=F/0=M)	0.59	0.5	0.57	0.5
Age	21.8	18.9	21.3	18.2
Under 10 (1=Yes)	0.42	0.5	0.44	0.5
Years of Education	4.1***	3.7	3.5	3.5
Household Head (1=Yes)	0.21	0.4	0.21	0.4
No. of HH Residents	5.6	1.8	5.7	2.6
No. of Rooms	2.1	7.9	2.0	8.1
Has Oportunidades (1=Yes)	0.78***	0.4	0.68	0.5
Has Insurance (1=Yes)	0.08**	0.3	0.06	0.2
In the last 30 Days:	**2002 Outcome Variables**			
Days Sick (Self Report)	1.5***	4.4	0.97	3.5
Days Sick (Activities)	0.53	2.4	0.62	3.5
Oupatient in Hospital?	0.03	0.2	0.02	0.1
Clinic Visit?	0.12***	0.3	0.07	0.3
Specialist Visit?	0.04***	0.2	0.01	0.1
Traditional Healer Visit?	0.004	0.1	0.01	0.1
Pharmacy Visit?	0.03***	0.2	0.01	0.1
Nurse Visit?	6x10-4	0.03	9X10-4	0.04
Any of the Above?	0.18***	0.4	0.11	0.3
Diabetes Test	0.10	0.3	0.10	0.3
Hypertension Test	0.2**	0.4	0.17	0.4
Days Spent in Hospital	0.13	1.1	0.11	0.8

*Asterisks denote significant differences between 2002 means for treated and control groups at the following levels: ***p<0.01, **p<0.05, *p<0.1*

Table 2 – Stepped wedge study design with two treatment effects

Cluster	Survey Year			
	2002	2003	2004	2009
Early Adopters (2004)	0	0	T1	T2
Late Adopters (2007-2009)	0	0	0	T1

Table 3 – Medium-term marginal effects of Seguro Popular on health utilization outcomes in last 30 days

In the last 30 days:	Full (N=4229)	Children (N=1096)	Women (N=1468)	Men (N=905)
Any clinic visit	0.03	0.05*	0.02	0.06
Standard error	0.04	0.03	0.06	0.05
z score	0.89	1.68	0.43	1.06
2002 mean	0.07	0.08	0.08	0.04
Any health visit	0.02	0.02	-0.02	0.06
Standard error	0.05	0.06	0.08	0.05
z score	0.40	0.39	-0.25	1.20
2002 mean	0.11	0.11	0.12	0.07
Total health visits	0.01	0.02	-0.08	0.04***
Standard error	0.05	0.03	0.10	0.01
z score	0.19	0.75	-0.82	3.73
2002 mean	0.16	0.16	0.20	0.10

*Mean treatment effect calculated according to equation 6 from coefficients resulting from random effects probit and Poisson regressions at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Regression standard errors clustered at municipio level. ***p<0.01, **p<0.05, *p<0.1*

Table 4 – Medium-term marginal effects of Seguro Popular on health outcomes in last 30 days

In the last 30 days:	Full (N=4229)	Children (N=1096)	Women (N=1468)	Men (N=905)
Days of illness (report)	-0.18	-0.12	-0.15	-0.16
Standard error	0.16	0.17	0.12	0.26
z score	-1.14	-0.70	-1.19	-0.62
2002 mean	0.97	0.52	1.55	0.95
Days of illness (activities)	-0.33*	-0.53**	-0.45**	0.13
Standard error	0.20	0.24	0.21	0.36
z score	-1.65	-2.27	-2.17	0.35
2002 mean	0.47	0.34	0.85	0.40

*Mean treatment effect calculated according to equation 6 from coefficients resulting from random effects Poisson regressions at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Regression standard errors clustered at municipio level. ***p<0.01, **p<0.05, *p<0.1*

Table A1 – Number of individuals in surveyed affected municipios in ENCELURB data, by year of affiliation

Treated 2009 (N=756)			Treated 2004 (N=3473)		
Number	State	Municipio	Number	State	Municipio
11	México	Hueypoxtla	106	Campeche	Carmen
3	México	Temascalapa	935	Chiapas	San Cristobal
47	México	Tenango	364	Chiapas	Tuxtla
31	México	Tenango	62	Guanajuato	Celaya
53	México	Tepetlixpa	198	Guerrero	Zihuatanejo
172	Michoacán	Apatzingan	22	Hidalgo	Tlaxcoapan
188	Tlaxcala	Contla de Juan Cuamatzi	117	México	Villa Guerrero
122	Veracruz	Coatepec	278	Morelos	Cuernavaca
18	Veracruz	Veracruz	96	Morelos	Emiliano Zapata
111	Veracruz	Xalapa	20	Morelos	Jiutepec
			129	Puebla	Amozoc
			98	Puebla	Tehuacán
			46	Veracruz	Agua Dulce
			42	Veracruz	Coatzacoalcos (1)
			114	Veracruz	Coatzacoalcos (2)
			153	Veracruz	Coatzacoalcos (3)
			193	Veracruz	Cosoleacaque (1)
			8	Veracruz	Cosoleacaque (2)
			442	Veracruz	Cosoleacaque (3)
			50	Veracruz	Ixhuatlán

Table A2 – Regression results from probit estimation of impact of Seguro Popular on likelihood of clinic visit in last month

VARIABLES	Full Sample	Children	Women	Men
One year post treatment	0.12	0.22	0.12	0.21
	[0.17]	[0.25]	[0.20]	[0.46]
Five year post treatment	0.45	0.74**	0.29	0.56
	[0.31]	[0.37]	[0.32]	[0.69]
Observations	16,916	4,384	5,872	3,620
Number of perid	4,229	1,096	1,468	905

*Result of random effects probit regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A3 – Regression results from probit estimation of impact of Seguro Popular on likelihood of any health care visit in last month

VARIABLES	Full Sample	Children	Women	Men
One year post treatment	0.15	0.10	0.07	0.29
	[0.16]	[0.25]	[0.17]	[0.33]
Five year post treatment	0.43	0.54	0.15	0.67
	[0.30]	[0.43]	[0.33]	[0.50]
Observations	16,916	4,384	5,872	3,704
Number of perid	4,229	1,096	1,468	926

*Result of random effects probit regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A4 – Results of Poisson estimation of impact of Seguro Popular on total health care visits in last month

VARIABLES	Full Sample	Children	Women	Men
One year post treatment	0.19	0.44	-0.09	0.88
	[0.23]	[0.48]	[0.25]	[0.59]
Five year post treatment	0.48	0.90	-0.13	1.62*
	[0.43]	[0.67]	[0.48]	[0.91]
Observations	16,916	4,408	5,872	3,704
Number of perid	4,229	1,102	1,468	926

*Result of random effects Poisson regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A5 - Results of Poisson estimation of impact of Seguro Popular on total reported days sick (activities) in last month

VARIABLES	Full Sample	Children	Women	Men
One year post treatment	-0.60*	-1.51**	-0.89*	0.54
	[0.35]	[0.71]	[0.50]	[0.94]
Five year post treatment	-1.05	-1.61**	-1.81*	0.69
	[0.72]	[0.79]	[1.03]	[1.39]
Observations	16,916	4,408	5,872	3,704
Number of perid	4,229	1,102	1,468	926

*Result of random effects Poisson regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A6 – Estimate of pre-trends in clinic usage in last 30 days in early adopter treatment group

VARIABLES	Full	Child	Women	Men
T-1	0.07	0.11	0.03	-0.02
	[0.12]	[0.14]	[0.19]	[0.24]
T	0.14	0.12	0.13	0.2
	[0.19]	[0.22]	[0.26]	[0.44]
T+5	0.36*	0.53**	0.18	0.36
	[0.19]	[0.22]	[0.22]	[0.24]
Early Adopters	-0.29*	-0.40**	-0.19	-0.14
	[0.17]	[0.19]	[0.19]	[0.20]
Observations	16,916	7,340	5,872	3,704
Number of perid	4,229	1,835	1,468	926

*Result of random effects probit regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A7 – Estimate of pre-trends in any health care usage in last 30 days in early adopter treatment group

VARIABLES	Full	Child	Women	Men
T-1	0.11	0.25	-0.03	-0.01
	[0.17]	[0.22]	[0.24]	[0.17]
T	0.20	0.29	0.05	0.29
	[0.21]	[0.21]	[0.27]	[0.35]
T+5	0.32	0.53**	0.06	0.38*
	[0.22]	[0.25]	[0.27]	[0.22]
Early Adopters	-0.37**	-0.54***	-0.19	-0.27*
	[0.16]	[0.18]	[0.18]	[0.14]
Observations	16,916	7,340	5,872	3,704
Number of perid	4,229	1,835	1,468	926

*Result of random effects probit regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of children under 6 in household, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A8 – Estimate of pre-trends in total number of health care visits in last 30 days in early adopter treatment group

VARIABLES	Full	Child	Women	Men
T-1	0.16	0.41	-0.18	0.19
	[0.28]	[0.39]	[0.37]	[0.41]
T	0.25	0.46	-0.19	0.95
	[0.32]	[0.34]	[0.37]	[0.66]
T+5	0.35	0.63*	-0.12	0.82**
	[0.31]	[0.33]	[0.35]	[0.38]
Early Adopters	-0.48**	-0.73***	-0.08	-0.62**
	[0.21]	[0.20]	[0.27]	[0.24]
Observations	16,916	7,340	5,872	3,704
Number of perid	4,229	1,835	1,468	926

*Result of random effects Poisson regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

Table A9 – Estimate of pre-trends in total number of health care visits in last 30 days in early adopter treatment group

VARIABLES	Full	Child	Women	Men
T-1	0.15	0.28	0.15	0.43
	[0.46]	[0.37]	[0.59]	[0.67]
T	-0.54	-1.13**	-0.86	0.76
	[0.36]	[0.57]	[0.61]	[0.79]
T+5	-0.40	-0.16	-0.92	0.38
	[0.45]	[0.39]	[0.60]	[0.58]
Early Adopters	0.09	-0.18	0.57	-0.58**
	[0.24]	[0.20]	[0.40]	[0.29]
Observations	15,609	6,033	5,872	3,704
Number of perid	4,229	1,835	1,468	926

*Result of random effects Poisson regression at individual level. Control variables include sex, age, age squared, education, number of household residents, number of rooms in house, dummy for speaking an indigenous language, status in the Oportunidades program, year, and dummy for 2004 access to Seguro Popular. Standard errors clustered at municipio level. Robust standard errors in brackets. ***p<0.01, **p<0.05, *p<0.1*

9. FIGURES

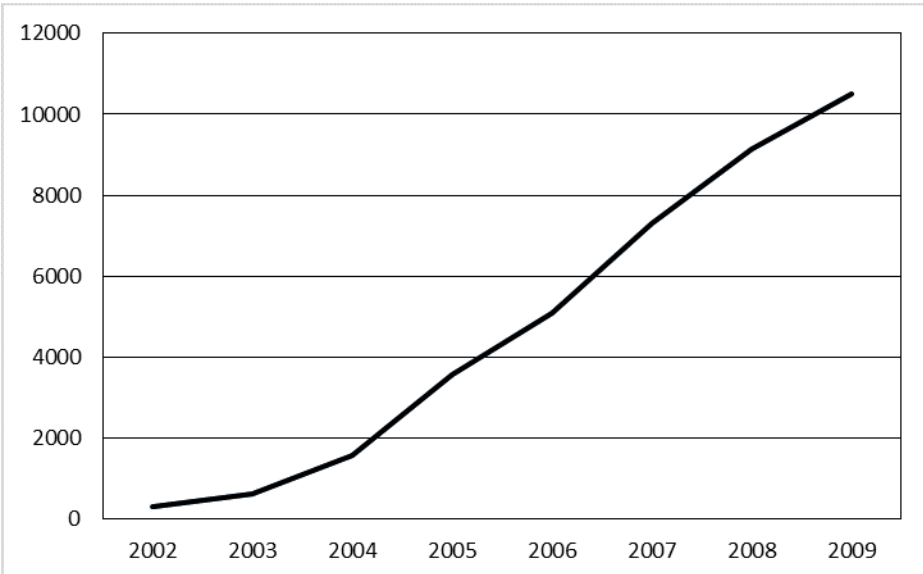

Figure 1 – Total Mexican families affiliated to Seguro Popular by year (in thousands). Data from Seguro Popular Administration. In 2002 and 2003, SP operated as a pilot program and 614,000 families were affiliated. The number of affiliated families rose to 1.7 million by the end of 2004; and by September of 2006, 4 million families were enrolled (Knaul et al. 2006). By 2012, 24 million families, or 52 million people, were affiliated (Knaul et al. 2012).

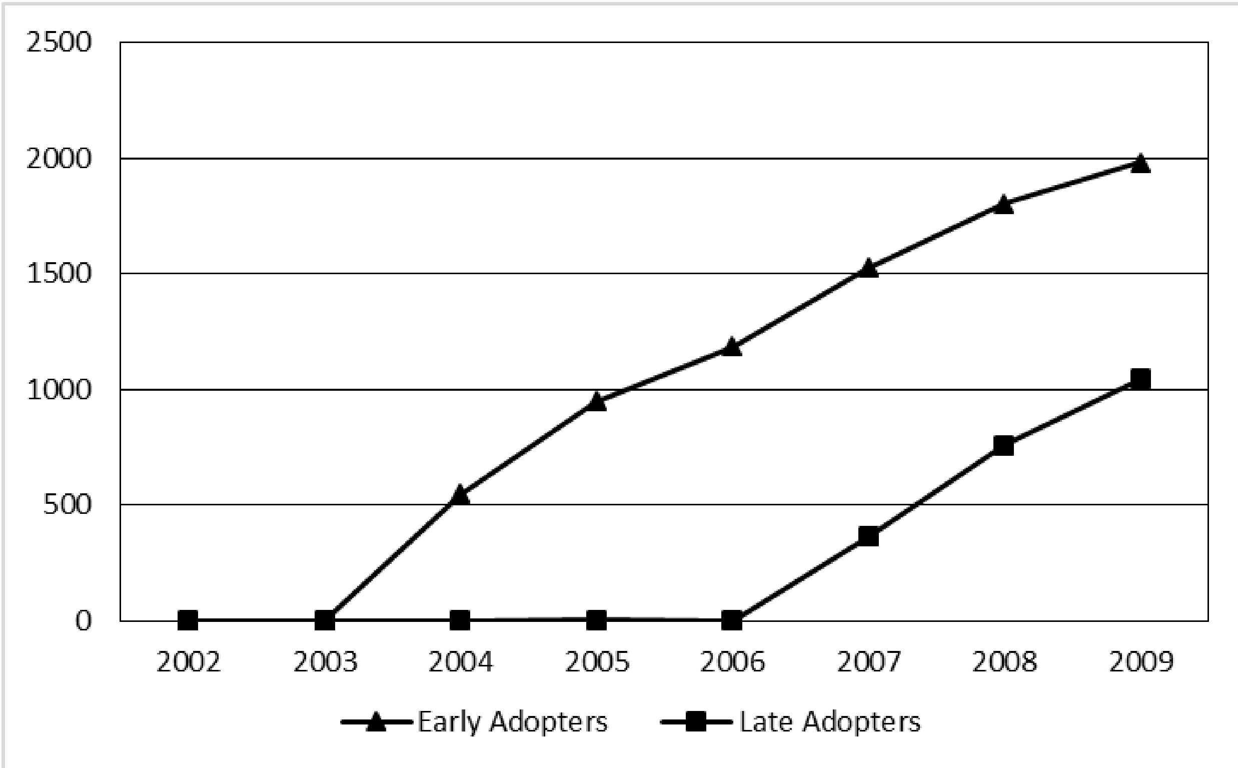

Figure 2 – Total Mexican affiliated families by year of accreditation in studied ENCELURB municipios. Data source is Seguro Popular administrative data.

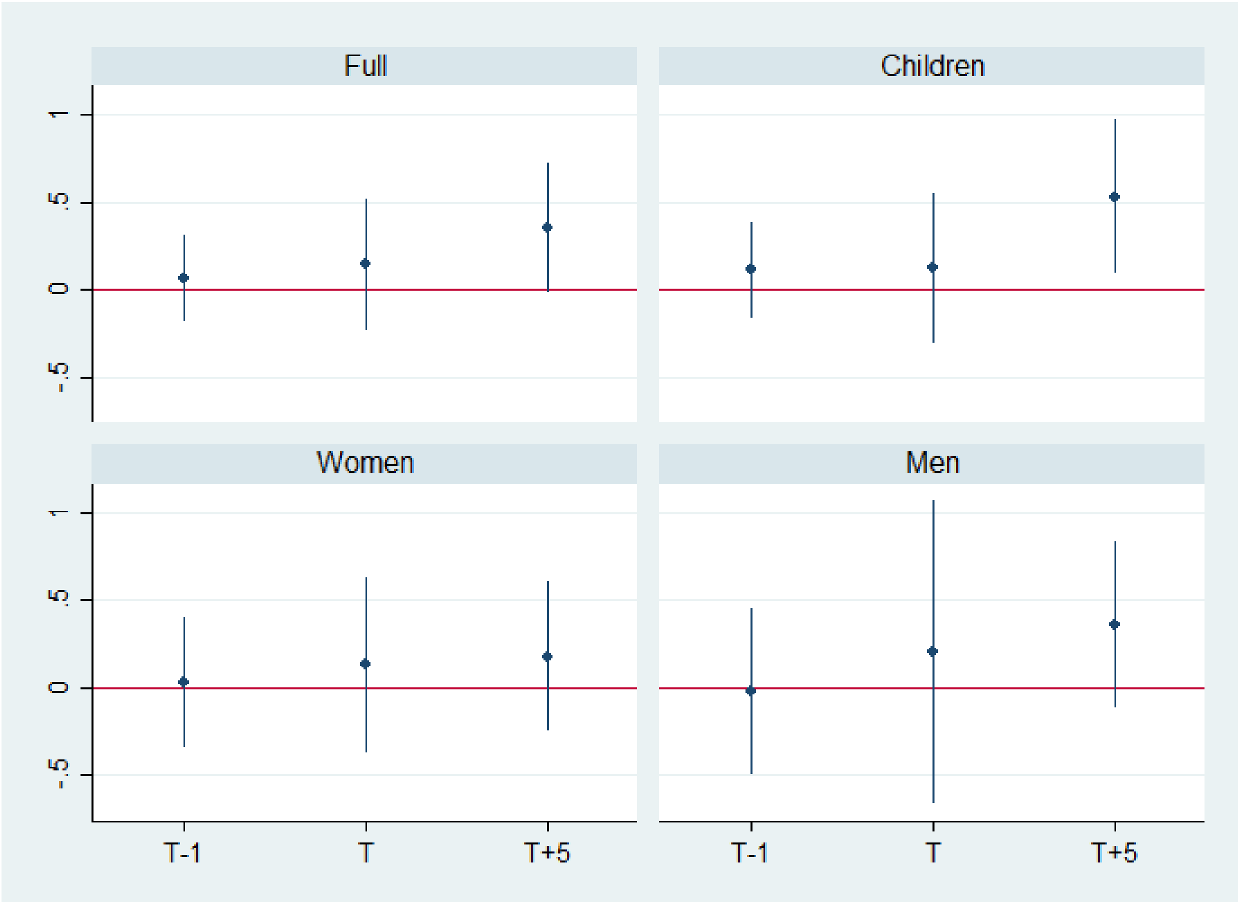

Figure A1 – Coefficients and confidence intervals for probit regression of pre and post-intervention trends on clinic usage in last 30 days for early adopter group with control variables and standard errors clustered at municipio level. T-1 shows impact one year before treatment (2003), T shows impact in treatment year (2004), and T+5 shows impact five years post treatment (2009).

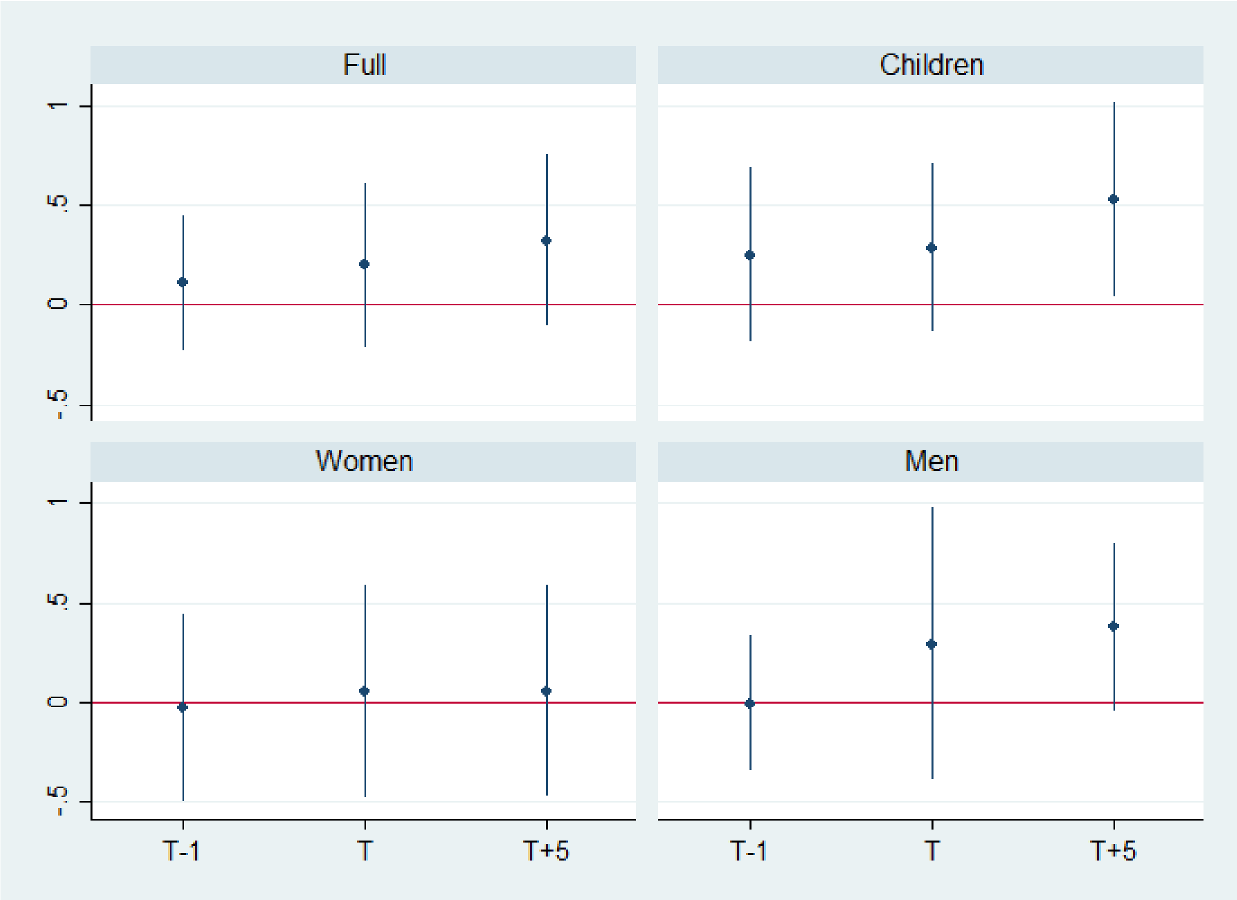

Figure A2 – Coefficients and confidence intervals for probit regression of pre and post-intervention trends on any health visit in last 30 days for early adopter group with control variables and standard errors clustered at municipio level. T-1 shows impact one year before treatment (2003), T shows impact in treatment year (2004), and T+5 shows impact five years post treatment (2009).

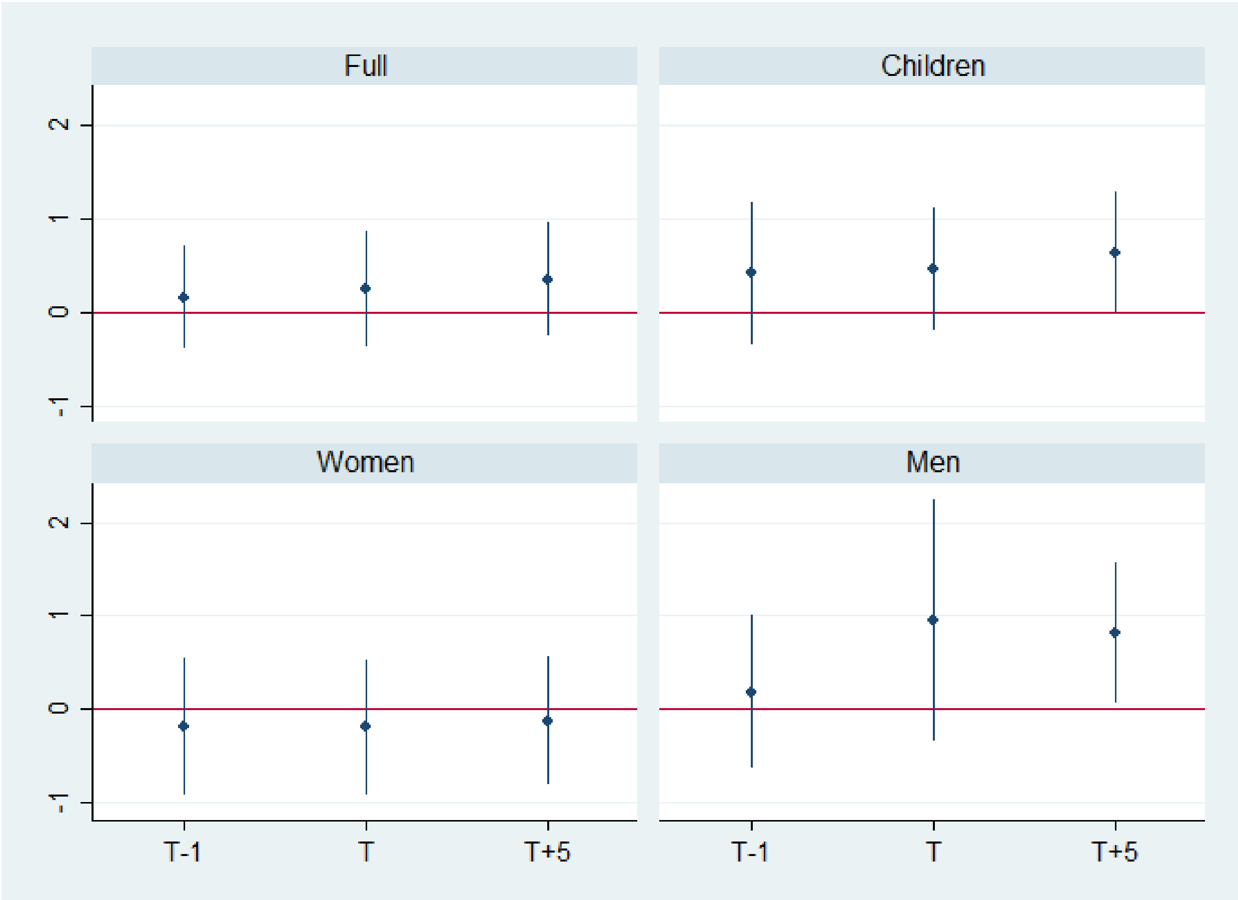

Figure A3 – Coefficients and confidence intervals for poisson regression of pre and post-intervention trends on total health visits in last 30 days for early adopter group with control variables and standard errors clustered at municipio level. T-1 shows impact one year before treatment (2003), T shows impact in treatment year (2004), and T+5 shows impact five years post treatment (2009).

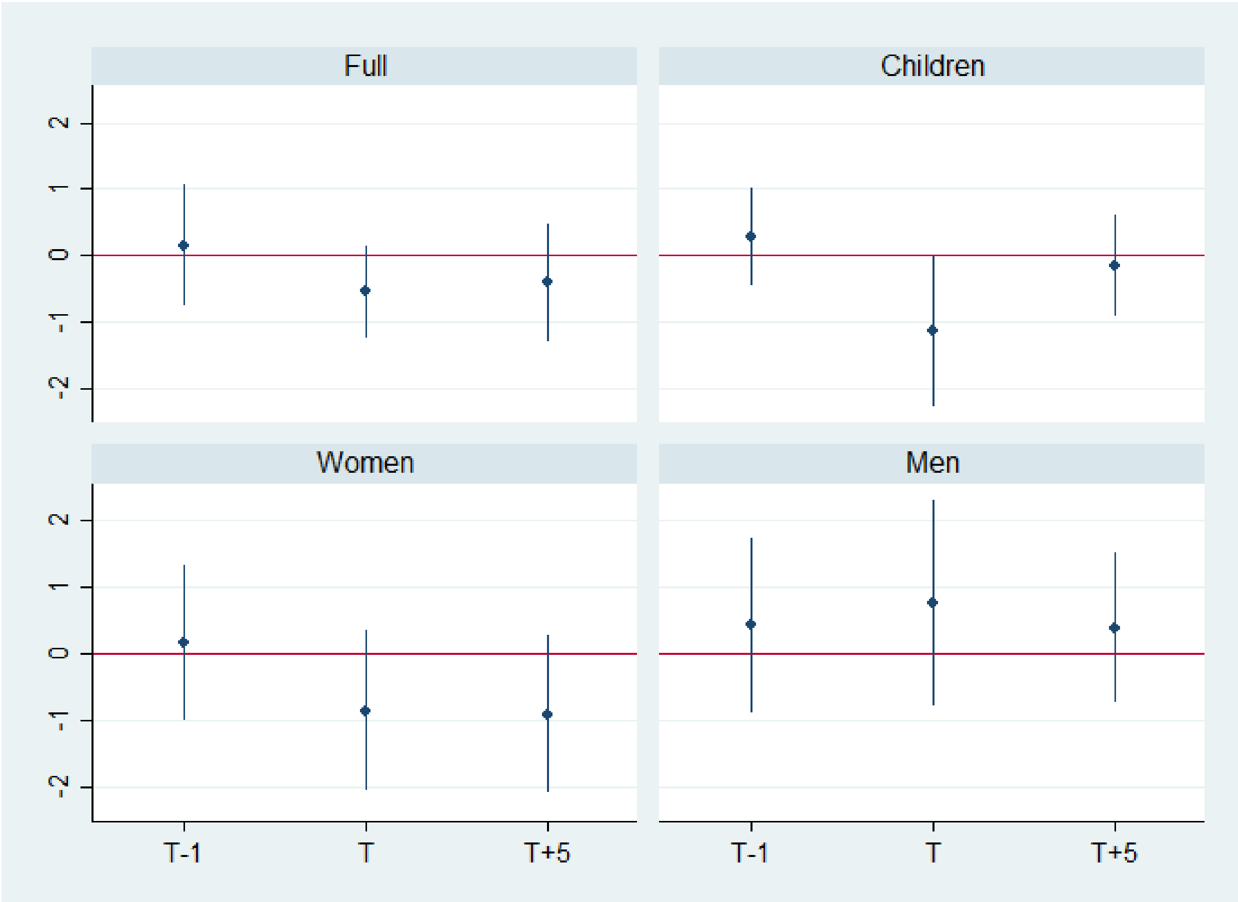

Figure A4 – Coefficients and confidence intervals for poisson regression of pre and post-intervention trends on total days sick (by activity) in last 30 days for early adopter group with control variables and standard errors clustered at municipio level. T-1 shows impact one year before treatment (2003), T shows impact in treatment year (2004), and T+5 shows impact five years post treatment (2009).